The discovery of cosmogenic $^{10}$Be in India

D. Lal

The search for beryllium-10 was an exciting mix of brilliant ideas, ingenious and heroic methods thousands of gallons of Bombay rain water and Himalayan snow melt—and tenacity.

The story of the discovery of cosmogenic $^{10}$Be produced in the earth's atmosphere is the story of independent evolution of scientific ideas in two groups in distant continents. This often happens in important scientific discoveries—and the discovery of $^{10}$Be was indeed an important milestone in nuclear geophysics/geochemistry. More than a dozen groups all over the world are now measuring the concentrations of $^{10}$Be in a wide variety of samples to learn about various parameters; past cosmic ray and geomagnetic field intensities, subduction of marine sediments along the plate margins and rates of erosion of natural surfaces, and many other leading questions in geosciences. I relate here the story of discovery of $^{10}$Be by B. Peters in India.

If the title of 'king' had to be given to a terrestrial cosmogenic nuclide (a nuclide produced by nuclear interactions of cosmic-ray particles with matter), it would no doubt go to $^{14}$C (half-life = 5730 yr), the very first to be discovered. By any standards, its detection was a brilliant accomplishment. The ratio $^{12}$C/$^{14}$C in modern carbon is ~$10^{-12}$. The detection$^4$ of $^3$H (half-life = 12.3 yr) by electrolytic enrichment was later accomplished by Libby and his colleagues for rain-water samples having $^3$H/$^2$H ratios of $\geq 10^{-18}$. This was another significant milestone in the field of cosmogenic nuclides. The third long-lived terrestrial cosmoneucleus to be detected was $^{10}$Be (half-life = 1.5 m.y.). Peters$^5$ discussed the potential applications of this nuclide in 1955. The nuclide was detected unambiguously and independently in 1956 in marine sediments by J. R. Arnold$^6$ in Chicago, and by B. Peters$^7$ and his colleagues in Bombay. Amongst the terrestrial atmospheric cosmogenic nuclides (henceforth called cosmoneuclei; cosmoneuclei for singular), $^{10}$Be occupies a high rank as a radiotracer because of its long half-life, 1.5 m.y. It is the longest lived of the terrestrial atmospheric radioactive cosmoneuclei and is useful for the study of processes and time-scales back to the Late Miocene. The detection of $^{10}$Be in the late fifties was therefore another milestone in the field of cosmogenic nuclides, but its studies, although the only means for determining accumulation rates of marine sediments and manganese nodules back to 10 m.y. in the past, remained confined to a few scientific groups in the world. This was a direct consequence of the fact that the measurement of $^{10}$Be activity involved very sophisticated radiochemical and low-level beta-counting methods. With the development of the AMS method 1977, leading to substantial improvements in the detection sensitivities, $^{10}$Be (and other long-lived nuclides) there was an almost immediate application explosion. This nuclide is being currently studied$^8$ in core samples of air, rain water, snow, rocks, ocean water, marine sediments, etc. to answer a wide range of questions in palaeoclimatology, glaciology, chronology, subduction of the sphere, geomagnetism and cosmo- physics. Studies of $^{10}$Be have been of industry. This nuclide ranks to the 'king', of cosmoneuclei, $^{12}$C.

The first detection of cosmogenic (terrestrial or extraterrestrial) was mainly accomplished by chemists; instance, of $^{14}$C by Libby. For Arrhenius, it would probably be natural and a relatively simple process to go after this nuclide! Peters was, however, a physicist. He launched a fledged attack to discover this nuclide after he had convinced himself that it was an important nuclide in view of chemical behaviour and long half-life when he published the first paper on $^{10}$Be in 1956. "Radioactive beryllium in the atmosphere and on the earth", he said:

It is estimated that about 1000 nuclide $^{10}$Be (27 m.y. half-life...
produced per square metre per second by cosmic-ray-induced nuclear disintegrations in
the atmosphere. The conditions for observing the resulting activity in rain water and in
various regions on the earth are favourable and may be useful for measuring sedimenta-
tion rates and other geological surface
changes during the Tertiary.

Peters then set out with full vigour to
detect this nuclide in snow, and in
marine sediments. He fully recognized
the importance of having at hand both
a first-class chemical procedure, and a
low-level beta-counting system. He con-
sulted with the best radiochemists in the
nuclear chemistry division of the Ato-
ic Energy Commission at Bombay.
These discussions soon convinced him
that the needed techniques were not
available. But Peters had his own way
of looking at the problem. He decided
that the task had to and could be
accomplished; and to those who showed
pessimism, he explained how to go
about it. He came up with explicit
suggestions in the field of radiochemi-
stry, an area of science entirely new to
him. His basic approach was correct,
but many of his ideas have still not
materialized. Nevertheless he soon suc-
cceeded in getting the chemists and
physicists working together and coming
up with a very specific chemical pro-
ducture which could extract $10^{12}$ atoms
of $^{10}$Be from a hundred grams of marine
sediments.

I was then a graduate student at the
Tata Institute of Fundamental Research
(TIFR), Bombay, and was working with
Peters, studying the primary cosmic
radiation, and the nature of character-
istics of elementary particles. In 1954,
Peters talked to us about his idea of
looking for $^{10}$Be in nature. $^{14}$C was
then a well-established fact. The global
mean production rate of $^{10}$Be was
estimated by Peters to be 0.1/cm² co-
lumsec, more than an order of
magnitude smaller than that of $^{14}$C.
Several senior scientists were most
sceptical of the $^{10}$Be idea of Peters: (i)
its small production rate, and (ii) finding
it in the depths of ocean sediments
where Peters claimed it would ultima-
tely find itself after production in the
atmosphere. I had then decided to work
on the $^{10}$Be project. Several scientists at
TIFR had severe doubts on Peters’ idea;
some even thought that he had proba-
bly cracked up, and told me so. Did I
believe in Peters’ ideas? I had no doubts
in my mind, and in fact I set out
immediately to do the experimental
work with other colleagues, P. S. Goel
and N. Narasappaya. My conviction
partly came about from the $^{10}$Be
production and deposition model clearly
set out by Peters, but more from the fact
that I believed that Peters must be right
because he says so — a sort of blind faith
in him. To me he had repeatedly
demonstrated that good science meant
thinking deep, trying to look deeper into
things, and then doing experiments to test
the ideas. It was then not important
whether finally one succeeded or not.
Some of us who were suitably exposed
to the Peters method of working had
become converts. It was a different
matter however whether they would
choose to work with him.

To fully appreciate my sentiments
and the blind (!) rationale of joining the
newly conceived $^{10}$Be project, it would
be useful to narrate some of my earlier
research work with Peters and how I
got conditioned to accepting his ideas.
I joined the cosmic-ray research group at
TIFR in late 1949. This group was
nicknamed the nuclear emulsion group
after the tool used to study cosmic rays.
H. J. Taylor of Wilson College, Bom-
bay, was then looking after the research
work of the group. A first-class physi-
cist, an outstanding teacher, he worked
hard and inspired us to study the nature
of cosmic-ray nuclear disintegrations
(stars, as they were called then) in
nuclear emulsions, a subject close to his
heart. About the same time, Peters had
some questions about the anti-matter
content of cosmic rays and decided that
the problem can best be explored by
exposing an E-W oriented package
of nuclear emulsions to cosmic radiation
at high altitude at equatorial Latitudes
(to eliminate the background due to low
energy primary cosmic-ray particles). He
conducted a series of balloon flights
jointly with Taylor in Madras during
October–November 1950 with the partici-
pation of several TIFR scientists,
including R. R. Daniel, M. S. Swami, Y.
Pal and myself. For me it was a great
experience, not just the flights but to see
first hand how basic research is done —
from an idea to its implementation.
And then a year later, as luck would
have it (for me at least), Peters joined
the TIFR towards the end of 1951 at
the invitation of H. J. Bhabha, Director,
TIFR. He then started a series of
exciting research projects which electrifi-
ced the intellectual portals of the
institute. First there was an intensive,
study of a high-energy meson shower in
a glass-backed nuclear emulsion plate;
the event was an interaction of a Mg
nucleus of kinetic energy, $7.8 \times 10^{15}$ eV/nucleon, producing more
than 350 charged and neutral mesons.
Both the event and the analysis were
first of their kind, leading to an insight
into nuclear interaction at ultra-high
energies.

At that time Peters was convinced
that the field of elementary particle
research then exclusively belonged to
cosmic-ray research, and proposed a
novel way of obtaining a large sample
of rare elementary particle events in
nuclear emulsions exposed to cosmic
rays. He proposed exposing and deve-
Sukumar Biswas

It was a cool, bright day in July 1952 when I met Bernard Peters for the first time in his well-organized office in the Old Yacht Club building of the Tata Institute of Fundamental Research. I had returned to India from my sojourn in Australia the previous day, and my first scientific interaction in Bombay on this day with two eminent scientists had a profound influence on my scientific career. In the meeting with Peters I spoke with much enthusiasm on our newly found exciting results "on the nuclear interactions of high-energy cosmic rays", which was the title of my Ph.D. thesis submitted to the University of Melbourne, Australia, in 1952. I was specially fond of one new result, which I explained to Peters — it was on a proton-proton collision at 1000 GeV, leading to multiple meson production. In this event we were able to measure directly, for the first time, the degree of inelasticity in p-p collision as 0.1 at 1000 GeV. This was measured in a very flat event in nuclear emulsion flown in a balloon in Australia and the primary proton energy was determined from Lorentz transformation in the centre-of-mass system. The secondary-particle energies were measured by careful multiple-scattering measurements in nuclear emulsion. These results, published in Physical Review in 1951, were one of the few that established the meson production as multiple and not plural. Peters showed great interest as, during the same time, the TIFR group observed a very-high-energy cosmic ray-Si interaction in emulsion, producing a giant meson shower of more than 300 particles. I discussed with him the new areas that I would like to explore in this field, and found enthusiastic support. My first meeting with him ensured my joining TIFR in 1952. Towards the end of my doctoral work in Australia I had a UNESCO fellowship from 1950, I wrote to my professor in Calcutta, M. N. Saha, about my interest in developing the new area of studies of high-energy cosmic rays with the newly developed technique of nuclear emulsions flown in balloons. I had earlier done doctoral work in nuclear physics in Calcutta University with Saha as my guide. Saha's reply was highly encouraging. He wrote that Bhabha has started this new area of research on a big scale under the direction of Peters and advised me to write to Bhabha. At that time, in 1950-52, Peters' name and fame were world-wide, and in Melbourne we were reading with great interest his epoch-making papers in Physical Review on the discovery of heavy nuclei in cosmic rays and their astrophysical implication. This work opened up a new dimension in cosmic-ray physics. From Melbourne I wrote a letter to Bhabha informing him of my new scientific findings in cosmic rays and of my keen interest to develop this new area with the new techniques of nuclear emulsions flown in balloons, and that the presence of Peters in Bombay gave special impetus to the idea. Bhabha replied in a telegram that I should see him on my arrival in Bombay. Thus, on the first morning in Bombay, I had the opportunity of discussing my research programme with Peters and Bhabha. My first meeting with Bhabha was a very enjoyable one as he enquired with keen interest about the details of the new results on high-energy interactions and how we obtained them. I explained to him how two of us, Hopper and myself, conducted balloon flights with meteorological balloons, tracked them with theodolites, processed the nuclear emulsions, scanned and analysed the data, and finally wrote the papers. Bhabha expressed much interest in our new results on the degree of inelasticity of high-energy interactions in nuclear emulsions as he himself was engaged in theoretical work on meson production. After about an hour of discussions, I thankfully accepted Bhabha's offer for me to join TIFR in Peters' group. A few months later, after a brief vacation and completion of formalities, I joined TIFR on 1 November 1952. I became close friends with the young team members of the nuclear emulsion group. Two of my colleagues of that time, Devendra Lal and Yash Pal, became my very active scientific collaborators and life-long friends. Soon discoveries were made of several striking events of heavy unstable particles produced in high-energy cosmic-ray interactions in a stripped emulsion stack. To make systematic and detailed studies of their diverse behaviour
 isotopic composition of lead at Cal-Tech during the early fifties, about
which he learned from Harrison Brown during one of his visits to TIFR. In any
case, Peters' $^{10}$Be idea started to jell some time in the late 1954, and he
decided that the first experiment to do should be the measurement of its
concentration in Kashmir snow. So we began chemical experiments on how to
extract $^{10}$Be from ca. 200,000 gallons of melt water at pH 2-3 using cation ex-
change resins.

Analogous to the heroic task of making an ideal nuclear emulsion block,
the task of $^{10}$Be chemistry and measurement confronted us with several pro-
blems. The task was probably much harder since a lot of micro-level radio-
chemistry was involved. Let me give you a flavour of some of the experiments
which were done. It was planned (more appropriately, proclaimed by Peters)
that nearly all the $^{10}$Be atoms present in 200,000 gallons of Kashmir snow melt
water would be first removed on some 50 kg cation resin; then after several
steps the same atoms would be concen-
trated on one resin particle of about 0.5
mm diameter, and then this resin bead
would be placed on a freshly poured
nuclear emulsion sheet (to ensure no
earlier background tracks) for a month
or so. Much of this was done, even fairly successfully. It was always a
challenge to try new things, however hard they may be. It was easier for some
of us to try out such ideas, especially those who had faith in what Peters
proclaimed. When he spoke, it looked
like a task which was just ready to be
done easily. Plans for the preparation
of large-glazed ceramic ion-exchange
columns and transporting the columns
with accessories (large porcelain sinks,
tubings, etc.) to Kashmir finally cul-
ninated in May, 1955. I was married on
17 May 1955 and within a week left for
Gulmarg, Kashmir, with my eighteen-
year-old bride, Aruna Lal. After accli-
matization at a lower altitude, we
camped at Khilanmarg (Kashmir). We
had four tents, one for the cook and
helpers, one for Peters, one for P. S.
Goel and B. S. Amin, and one for Aruna Lal and myself. The experiment began
soon enough, all as planned, but then
within a week, Peters decided to leave
alone for Bombay, to catch the first
rains, and to measure the concentration of the cosmic-ray-produced $^7$Be in the
rain water. We continued the $^{10}$Be
extraction experiment from Khilan-
marg snows.

$^7$Be was detected$^{14}$ during the Bom-
bay monsoon. The work on $^{10}$Be

properties, Peters organized two parallel groups—one, with Lal, Pal and Peters, was working on production and decay properties; the other, with myself, E. C. George (later E. C. G. Sudarshan) and Peters, on their mass measurements. We were engrossed in hectic work during these first studies in a stripped emulsion stack, and after six to seven months of exciting work in November 1952–June 1953, a series of four
spectacular papers from the nuclear emulsion group were presented at the third international cosmic ray conference at Baghières de Bigorre, France, in July 1953 by Peters. Bhambha
also attended the conference. Immediately the TIFR emulsion group achieved international fame and these papers were
referred to all over the world. These new studies were later published in detail as companion papers in the Proceedings
of the Indian Academy of Sciences A, in November 1953—

![Nikita Kruchëev observing a balloon-flown nuclear emulsion containing the record of a very-high-energy cosmic-ray intersection, TIFR, about 1955. President Bulgarin is also seen, second from right. Also in the picture are, from left, Bernard Peters, Homi Bhabha, Sukumar Biswas, the Soviet interpreter, K. A. Neelakantan, P. J. Lavakare (behind Biswas) and P. S. Goel (behind the interpreter)](image)

Nikita Kruchëev observing a balloon-flown nuclear emulsion containing the record of a very-high-energy cosmic-ray interaction, TIFR, about 1955. President Bulgarin is also seen, second from right. Also in the picture are, from left, Bernard Peters, Homi Bhabha, Sukumar Biswas, the Soviet interpreter, K. A. Neelakantan, P. J. Lavakare (behind Biswas) and P. S. Goel (behind the interpreter)

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one by Lal, Pal and Peters, Observations on $\tau$-mesons and
on K-mesons giving rise to capture stars (ibid, 1953, 38, 398); and the other by Biswas, George and Peters, An improved method for determining the mass of particles from scattering vs range and application to the mass of K-mesons' (ibid. 1953, 38, 418). In the latter paper I had the good fortune of applying my earlier knowledge and experience of multiple-scattering measurements to discover a new method of constant sagitta scattering, and this has become a standard method followed in all the laboratories of the world in the following decades. Using this new method we were able to show in 1953, for the first time, that the masses of the $\tau$-
meson and $K^+$- and K-mesons were the same, 974 $\pm$ 2 MeV,
in contrast to the different masses suggested by the UK and
other groups. All these studies bear the stamp of the methodical planning of Peters and of his directions and
guidance for their meticulous execution in all the details of the problem. These, together with the support of a highly
dedicated team of young scientists, enabled the nuclear
emulsion group of TIFR to be one of the top-ranking in the
world. Soon afterwards, the work of the group diversified in
many areas.

I had the good fortune to be intimately associated with
Peters, and I acknowledge learning from him the scientific
methodology of how a complex scientific problem can be
solved by subdividing it into separate individual components
and pursuing these to their logical conclusions. My close
association with him continued till his departure from India in
1959; outside work my wife and I had a very friendly and
cordial relationship with him and his family. On both scientific
and personal planes, I have continued to have a most cordial
relationship with Peters and his family in Denmark. On
invitation from him I visited, a few times, his laboratories at the
Danish Space Research Institute in Copenhagen for scientific
lectures and discussions and I greatly benefited from them
and enjoyed his excellent hospitality.
learned about Peters’ ideas on $^{10}\text{Be}$ from Prof. S. Chandrasekhar. Before this time, Arnold had not seen Peters’ paper in the *Proceedings of the Indian Academy of Sciences.*

$^{10}\text{Be}$ was detected unambiguously in 1956 by the two groups in sediments from the Eastern Pacific Ocean. Arnold analysed seven samples of 0–120 cm depth from two cores and Peters’ group in four upper and lower sections of a 15-m-long core. The $^{10}\text{Be}$ activity was shown to follow $\text{Be}$ chemistry by the two groups. The energy of the beta radiation was measured by both the groups by absorption measurements. Arnold showed that the measured $^{10}\text{Be}$ concentrations in his samples were in accord with its production estimates and sedimentation rate of $\sim 1 \text{mm/}^{10}\text{Be}\text{yr}$. The results of the Bombay group for samples separated by $\sim 12 \text{m}$ allowed estimation of sedimentation rate, $^{10}\text{Be}$ deposition rate and limits on temporal changes in the cosmic ray intensity in the past 1.6–3.2 m.y. Peters was quite excited about the potential of $^{10}\text{Be}$ for determining both the chronology of marine sediments as well as changes in the terrestrial cosmic-ray flux, and had elaborated on the application of the $^{10}\text{Be}$ method in a second paper on the subject. We of course know now that temporal and spatial fluctuations in some geochemical/geophysical parameters preclude a straightforward application of the $^{10}\text{Be}$ tracer method. However, the approach taken by Peters illustrates the method of his work. To most scientists in his situation, it would have been sufficient to first detect $^{10}\text{Be}$, and then to think of the next step. The problem of detection of $^{10}\text{Be}$ was indeed hard—both on accounts of radiochemical and radiation measurement requirements. For the Bombay group, radiochemistry was quite a challenge; even Arnold, a professional, has remarked that isolation of $^{10}\text{Be}$ was a difficult task.

The detection of natural $^{7}\text{Be}$ and $^{10}\text{Be}$ activities at Bombay produced great excitement in the scientific community. Independent search in the two continents for natural $^{10}\text{Be}$ added both spice and a comradeship. It is interesting to comment here that Arnold decided to look first for natural $^{7}\text{Be}$ and then $^{10}\text{Be}$; a logical thing to do since the former was easier. Peters, on the other hand, looked for $^{10}\text{Be}$ first. He was convinced that it was the most important thing to do, but later on looked for $^{7}\text{Be}$, to use it also for ascertaining the production rate of $^{10}\text{Be}$.

It should be mentioned here that the half-life of $^{10}\text{Be}$ was earlier believed to be 2.5 m.y., a value given by Macmillan. Discrepancies in the $^{10}\text{Be}$ cross-sections measured by counting $^{10}\text{Be}$ beta activity and by mass-spectrometer led Yiou and Raisbeck to check on its half-life. They revised its half-life to 1.4 m.y. Later Macmillan explained that an oversight the value he published earlier was the mean-life of $^{10}\text{Be}$ and not the half-life. Introducing the factor of log 2 would make his half-life estimate to be 1.1 m.y., in good agreement with the value of 1.5±0.3 m.y. published by Yiou and Raisbeck.

Peters worked in India at a time when national fervour for science was high. Science was being nurtured by Nehrni, Bhabha, Bhatnagar and others. Encouragement was all there, so that any idea however costly could be undertaken! Several young and bright scientists were looking for opportunities to work on good ideas. Peters provided this opportunity to some of us. As you can see, I was one of those who grabbed this tightly. I graduated from a microscope to radiochemistry and low-level counting—with varied experiences on the side: balloon flights, leopard hunt, etc. Peters made a severe impact on the scientific community at TIFR (and in India as a whole). It was often very difficult to work with him (an understatement): he had too many ideas and demanded immediate attention. However those of us who decided to work closely with him have reaped a life-long benefit. We try to emulate him, and work hard ourselves and with our colleagues. The story of detection of $^{10}\text{Be}$...
in India is not an unusual story. Such things happen all the time in science. For me and many of my colleagues, for instance Pal, Goel and Rama, it demonstrated that we could do good science in India if we had confidence in ourselves.


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